strength of its tendency to regress; thus a mean regression from 1 in the mid-parents to ⅔ in the offspring would indicate only half as much stability as if it had been to ⅓.

The mean regression in stature of a population is easily ascertained, but I do not see much use in knowing it. It has already been stated that half the population vary less than 1·7 inch from mediocrity, this being what is technically known as the "probable" deviation. The mean deviation is, by a well-known theory, 1·18 times that of the probable deviation, therefore in this case it is 1·9 inch. The mean loss through regression is ⅓ of that amount, or a little more than 0·6 inch. That is to say, taking one child with another, the mean amount by which they fall short of their mid-parental peculiarity of stature is rather more than six-tenths of an inch.

With respect to these and the other numerical estimates, I wish emphatically to say that I offer them only as being serviceably approximate, though they are mutually consistent, and with the desire that they may be reinvestigated by the help of more abundant and much more accurate measurements than those I have had at command. There are many simple and interesting relations to which I am still unable to assign numerical values for lack of adequate material, such as that to which I referred some time back of the superior influence of the father over the mother on the stature of their sons and daughters.

The limits of deviation beyond which there is no regression, but a new condition of equilibrium is entered into, and a new type comes into existence, have still to be explored. Let us consider how much we can infer from undisputed facts of heredity regarding the conditions amid which any form of stable equilibrium, such as is implied by the word "type," must be established, or might be disestablised and superseded by another. In doing so I will follow cautiously along the same path by which Darwin started to construct his provisional theory of pangenesis; but it is not in the least necessary to go so far as that theory, or to entangle ourselves in any questioned hypothesis.

There can be no doubt that heredity proceeds to a considerable extent, perhaps principally, in a piecemeal or piebald fashion, causing the person of the child to be to that extent a mosaic of independent ancestral heritages, one part coming with more or less variation from this progenitor and another from that. To express this aspect of inheritance, where particle proceeds from particle, we may conveniently describe it as "particulate."

So far as the transmission of any feature may be regarded as an example of particulate inheritance, so far (it seems little more than a truism to assert) the element from which that feature was developed must have been particulate also. Therefore, wherever a feature in a child was not personally possessed by either parent, but transmitted through one of them from a more distant progenitor, the element whence that feature was developed must have existed in a particulate, though impersonal and latent, form in the body of the parent. The total heritage of that parent will have included a greater variety of material than was utilised in the formation of his own personal structure. Only a portion of it became developed; the survival of at least a small part of the remainder is proved, and that of a larger part may be inferred by his transmitting it to the person of his child. Therefore the organised structure of each individual should be viewed as the fulfilment of only one out of an indefinite number of mutually exclusive possibilities. It is the development of a single sample drawn out of a group of elements. The conditions under which each element in the sample became selected are, of course, unknown, but it is reasonable to expect they would fall under one or other of the following agencies: first, self-selection, where each element selects its most suitable neighbour, as in the theory of pangenesis; secondly, general co-ordination, or the influence exerted on each element by many or all of the remaining ones, whether in its immediate neighbourhood or not; finally, a group of diverse agencies, alike only in the fact that they are not uniformly helpful or harmful, that they influence with no constant purpose—in philosophical language, that they are not teleological; in popular language, that they are accidents or chances. Their inclusion renders it impossible to predict the peculiarities of individual children, though it does not prevent the prediction of average results. We now see something of the general character of the conditions amid which the stable equilibrium that characterises each race must subsist.

Political analogies of stability and change of type abound, and are useful to fix the ideas, as I pointed out some years ago. Let us take that which is afforded by the government of a colony which has become independent. The individual colonists rank as particulate representatives of families or other groups in the parent country. The organised colonial government ranks as the personality of the colony, being its mouthpiece and executive. The government is evolved amid political strife, one element prevailing here and another there. The prominent victors band themselves into the nucleus of a party, additions to their number and revisions of it ensue, until a body of men are associated capable of conducting a completely organised administration. The kinship between the form of government of the colony and that of the parent state is far from direct, and resembles in a general way that which I conceive to subsist between the child and his mid-parentage. We should expect to find many points of resemblance between the two, and many instances of great dissimilarity, for our political analogy teaches us only too well on what slight accidents the character of the government may depend when parties are nearly balanced.

The appearance of a new and useful family peculiarity is a boon to breeders, who by selection in mating gradually reduce the preponderance of those ancestral elements that endanger reversion. The appearance of a new type is due to causes that lie beyond our reach, so we ought to welcome every useful one as a happy chance, and do our best to domicile and perpetuate it. When heredity shall have become much better and more generally understood than now, I can believe that we shall look upon a neglect to conserve any valuable form of family type as a wrongful waste of opportunity. The appearance of each new natural peculiarity is a faltering step in the upward journey of evolution, over which, in outward appearance, the whole living world is blindly blundering and stumbling, but whose general direction man has the intelligence dimly to discern, and whose progress he has power to facilitate.

THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE[1]

THE meeting of 1885 of the American Association for the Advancement of Science was held at the Ann Arbor University. The total attendance (according to *Science*) of members was not a large one, the number reaching only to 365; the number of papers was 176. Two changes in the organisation were made; by one, the section of histology and microscopy was abolished, as it has been urged for some time that a special science of microscopy does not exist, the microscope being rather a tool to be used by scientific men in various branches. The other change was in the name of the section of mechanics, the words "and engineering" being added to the title, that it may be more clearly understood by Americans that those interested in all branches of engineering are invited to take part in the proceedings. As this was the first meeting since the action of the Government in regard to the Coast Survey, the question was generally discussed. The matter was referred to a committee, which offered to a general session of the Association the following resolutions, which were unanimously accepted:—

WHEREAS, The attention of this Association has been called to articles in the public press, purporting to give—and presumably by authority—an official report of a Commission appointed by the Treasury department to investigate the condition of the U.S. Coast Survey Office, in which report the value of a certain scientific work is designated as "meagre."

AND WHEREAS, This Association desires to express a hope that the decision, as to the utility of such scientific work, may be referred to scientific men.

Resolved, That the American Association for the Advancement of Science is in earnest sympathy with the Government in its every intent to secure the greatest possible efficiency of the public service.

Resolved, That the value of the scientific work performed in the various departments of the Government can be best judged by scientific men.

Resolved, That this Association desires to express its earnest approval of the extent and high character of the work performed by the U.S. Coast Survey—especially as illustrated by the gravity determinations now in progress—and to express the hope that such valuable work may not be interrupted.

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[1] For early copies of the addresses and papers we are indebted to the editor of *Science*.

Resolved, That this Association expresses, also, the hope that the Government will not allow any technical rule to be established that shall necessarily confine its scientific work to its own *employés*.

Resolved, That in the opinion of the American Association for the Advancement of Science, the head of the Coast Survey should be appointed by the President, by and with the advice and consent of the Senate, should have the highest possible standing among scientific men, and should command their entire confidence.

Resolved, That copies of these resolutions shall be prepared by the general secretary, and certified by the President of the Association and by the permanent secretary, and shall be forwarded to the President of the United States, the Secretary of the Treasury, and given to the press.

Various improvements with the object of securing a more rapid despatch of business were either suggested or adopted; thus members are to be elected by a standing committee instead of in general session, and it is proposed to restrict general sessions of the Association to the beginning and close of the meeting, and to limit the public reading of committee reports in general session to such as seem to the standing committee specially desirable from their interest or importance. The next meeting will be held at Buffalo, beginning August 18, 1886, under the presidency of Prof. Edward S. Morse, of Salem.

We regret much that it is impossible for us to reproduce in full the President's address and the sectional reports; the obvious pressure on our space at the present time will only enable us to refer to a few salient topics. The President's address was delivered by Prof. J. P. Lesley, of Philadelphia. We find the following striking observations on the "dead-work" of science:—

There is a topic which I think should be frequently considered by all who engage in scientific pursuits, and by none so earnestly as by those who are ambitious to reach the higher points of view, from which to survey and describe those systematic combinations of phenomena which are more or less panoramic: I allude of course to generalisers or discoverers of natural laws, and the professional teachers of such laws: while those who deal in itemised science, the mere observers of isolated facts, discriminating specimens and naming genera and species in the animal, vegetable, or mineral worlds, and especially such as occupy themselves with geographical and geological studies in detail, stand in less need of having it pressed upon their attention, because in their case it insists upon its own necessity.

I allude to what is technically known among experts as "dead-work."

This topic has to be treated in the most prosaic style. To describe dead-work is to narrate all those portions of our work which consume the most time, give the most trouble, require the greatest patience and endurance, and seem to produce the most insignificant results. It comprises the collection, collation, comparison and adjustment, the elimination, correction, and reselection, the calculation and representation—in a word, the entire first, second, and third handling of our data in any branch of human learning—wholly perfunctory, preparatory, and mechanical, wholly tentative, experimental, and defensive—without which it is dangerous to proceed a single stage into reasoning on the unknown, and futile to imagine that we can advance in science ourselves, or assist in its advancement in the world. It is that tedious, costly, and fatiguing process of laying a good foundation which no eye is ever to see, for a house to be built thereon for safety and enjoyment, for public uses or for monumental beauty. It is the labour of a week to be paid for on Saturday night. It is the slow recruiting, arming, drilling, victualling, and transporting of an entire army to secure victory in one short battle. It is the burden of dead weight which every great discoverer has had to carry for years and years, unknown to the world at large, before the world was electrified by his appearance as its genius. Let us examine it more closely: it will repay our scrutiny. Those of you who have been more or less successfully at work all your lives may get some satisfaction from the retrospect: and those who have commenced careers should hear what dead-work means, what its uses are, how indispensable it is, how honourable it is, and what stores of health and strength and happiness it reserves for them.

My propositions, then, are these:—(1) That, without a large amount of this dead-work, there can be no discovery of what is rightly called a scientific truth. (2) That, without a large amount of dead-work on the part of a teacher of science, he will fail in his efforts to impart true science to his scholars. (3) That, without a large amount of dead-work, no professional expert can properly serve, much less inform and command, his clients or employers. (4) That nothing but a habitual performance of dead work can keep the scientific judgment in a safe and sound condition to meet emergencies, or prevent it from falling more or less rapidly into decrepitude; and (5) That in the case of highly-organised thinkers, disposed or obliged to exercise habitually the creative powers of the imagination, or to exhaust the will-power in frequently-recurring decisions of difficult and doubtful questions, dead-work and plenty of it is their only salvation; nay, the most delicious and refreshing recreation; a panacea for disgust, discouragement, and care; an elixir vitæ; a fountain of perpetual youth. . . .

First, then, is it so that scientific truths cannot be discovered without a large amount of preliminary dead-work? Surely no one in this assembly doubts it who has established even one original theory for himself, or won for it the suffrages of judges capable of weighing evidence. Now the immense disproportion in numbers between theories broached and theories accepted is the best proof we could have, not only of the value and necessity of dead-work, but of the scarcity of those who depend upon it as a preparatory stage of theorising. And, moreover, not theories only, but simple statements of fact believed and disbelieved—that is, finally accepted or finally rejected—exhibit the like numerical disproportion, and betray a general carelessness or laziness of observers; at all events their manifest lack of appreciation of the value and necessity of the dead-work part of observation, which imperatively must precede any clear mental perception of the simplest phenomenon, before the attempt is made to establish its natural relationships, and present it for acceptance as a part of science.

No; dead work cannot be delegated. The man who cannot himself survey and map his field, measure and draw his sections properly, and perfectly represent with his own pencil the characteristic variations of its fossil forms, has no just right to call himself an expert geologist. These are the badges of initiation; and the only guarantees which one can offer to the world of science that one is a competent observer and a trustworthy generaliser. Nor has one become a true man of science until he has already done a vast amount of this dead work; nor does one continue in his prime, as a man of science, after he has ceased to bring to this test of his own ability to see, to judge, and to theorise, the working and thinking of other men. But enough of this.

My second proposition was that no teacher of science can be successful who does not himself encounter some of the dead work of the explorer and discoverer; who does not discipline his own faculties of perception, reflection, and generalisation, by field-work and office-work, independently of all text-book assistance; who does not himself make at least some of the diagrams, tables, and pictures for his class-room, in as original a spirit and with as much precision of detail as if none such had ever been made before, and these were to remain sole monuments of the genius of investigation. What the true teacher has to do first and foremost is to wake up in youthful minds this spirit of investigation *ab initio*. The crusade against scholastic cramming promises to be successful; but the crusade against pedagogic cramming has hardly yet been organised. How is the scholar to be made an artist if the teacher cannot draw? The instinct of imitation in man is irresistible. Slovenly drawing on the blackboard—sufficient evidence of the teacher's imperfect information and inaccurate conception of facts, the nature of which he only thinks he understands—can do little more than raise a cold fog of suspicion in the class-room, by which the tender sprouts of learning must be either dwarfed or killed. But even slovenly diagrams are preferable to purchased ones; for whatever diminishes the dead-work of a teacher enervates his investigating, and thereby his demonstrating, powers, and lowers him toward the level of his scholars.

Were I dictator I should drive all teachers of science out into the great field of dead-work; force them to go through all the gymnastics of original research and its description, and not permit them to return to their libraries until their notebooks were full of their own measurements and calculations, sketch-maps and farm-drawings, severely accurate and logically classified, to be then compared with those recorded in the books. What teachers fail to keep in mind is this: that learning is not knowledge; but as Lessing says: Learning is only our knowledge of the experience of others; knowledge is our own. No man really comprehends what he himself has not created. Therefore we know nothing of the universe until we take it to pieces for inspection, and rebuild it for our understanding. Nor can one man do this for another; each must do it for himself; and all that one can do to help another is to show him how he himself has morsellated and recomposed his small particular share of concrete nature, and inspire him with those vague but hopeful suggestions of ideas which we call learning, but which are not science.

My third proposition was that an expert in practical science can command the respect and confidence of his professional fellows, and through their free suffrages build up his own reputation in the learned and business worlds, only in exact proportion to the amount of good dead-work to which he voluntarily subjects himself. For, although the most of it is necessarily done in secrecy and silence, enough of it leaks out to testify to his honest and diligent self-cultivation; and enough of it must show in the shape of scientific wisdom to make self-evident the fact that he is neither a tyro nor a charlatan. More than once I have heard the merry jest of the Australasian judge quoted with sinister application to experts in science. When a young colleague, just arrived from England, asked him for advice, he answered: Pronounce your decisions, but beware of stating your reasons for them. Many an ephemeral reputation for science has been begot by this shrewd policy; but the best policy to wear well is honesty; and honesty in trade means selling what is genuine, well-made, and durable; and honesty in science means, first, facts well proved, and then conclusions slowly and painfully deduced from facts well proved, in sufficient number and order of arrangement to exhaust alike the subject and the observer. Reap your field so thoroughly that gleaners must despair. Fortify your position, that your most experienced rival can find no point of attack. Lay your plans with such a superfluity of patient carefulness that fate itself can invent no serious emergency. Demonstrate your theory so utterly and evidently that it shall require no defender but itself. Die for your work, that your work may live for ever. Forget yourself, and your work will make you famous. Enslave yourself to it, and it will plant your feet upon the necks of kings, and your mere Yes or No will become a law to multitudes. This is what the dead-work of science, when well done, does for the expert in science.

My fourth proposition—that only the habitual performance of dead-work can preserve the scientific intellect in pristine vigour, and prevent it from becoming stiffened with prejudices, inapt to receive fresh truth, and forgetful of knowledge already won—hardly needs discussion. Human muscles become atrophied by disuse. Men's fortunes shrink and evaporate by mere investment. I pray you to imagine what I wish to say, for it all amounts to this—that the grass will surely grow over a deserted footpath. Let me hurry to the close of this address, which I have found too serious a duty for my liking, and perhaps you also have found it too personal a preachment for yours. One more suggestion, then, and I have done.

My fifth proposition was that the wearied and exhausted intellect will wisely seek refreshment in dead-work.

The physiology of the brain is now sufficiently well understood to permit physicians to prescribe with some assurance for its many ills, and to regulate its restoration to a normal state of health. Its tissues reproduce themselves throughout life if no extraordinary over-balance of decay takes place, if there be no excessive and too long-continued waste. For the majority of mankind, nature provides for the adjustment between consumption and reproduction of brain matter, by the alternations of day and night, noise and silence, society and solitude; and also by the substitution of the play of fancy in dreams, for the work of the judgment and the will in waking hours. We follow the lead of nature when we seek amusement as a remedy for care. We bring into activity a rested portion of the brain, to permit the wearied parts of it to restore themselves unhindered.

In Section A Prof. Newton, of Yale, read a paper upon "The Effect of Small Bodies passing near a Planet upon the Planet's Velocity."

The former researches of Prof. Newton upon meteors are recognised among astronomers as our principal source of knowledge about the character, distribution, and motion of these minute bodies with which the solar system is filled, especially those which strike our atmosphere and are burned up as meteors. The possible effect of these upon the rotation of the earth, and the revolution of the earth and moon in their orbits, has been subjected to elaborate investigation at the hands of several mathematical astronomers. The recent publications of Mr. Denning, of Bristol, claiming the fixity of long-continuing radiant points of meteor streams, have raised the question of the existence of broad streams of meteoroids moving swiftly through stellar space outside of solar attraction; and any new investigation bearing upon any of these points is more than usually timely. In this paper Prof. Newton has discussed the effect upon the earth's motion of those bodies which do not pass near enough to the earth to be drawn into its atmosphere, but still near enough to be drawn out of their course, and swung for a time in hyperbolic orbits around it. He began by saying that the results of the investigation might perhaps be considered negative as far as measurable quantities in the solar system are concerned, but that they had a mathematical interest, and might possibly have a bearing upon somewhat similar questions in molecular physics, like the kinetic theory of gases. The mathematician and astronomer must be referred to the paper itself, and the results of popular interest may be briefly summarised as follows:—Considering, first, the case of a cylindrical stream of small bodies evenly distributed, and all moving in the same direction with a common velocity past the earth supposed to be in the axis of the cylinder, it is shown that they will communicate to the earth in each unit of time a velocity along the axis: (1) that is proportional to the density of the group; (2) that decreases as the velocity increases nearly inversely as the square of the velocity; (3) that increases as the logarithm of the radius of the cylinder, the radius being measured by a unit differing from the earth's radius by a small quantity, which is a function of the velocity. Second, in the case of a widely-extended group of small bodies evenly distributed in space, and having speeds all equal, but directed towards points evenly distributed over the celestial sphere with the earth moving in a right line through them, it is shown that, for those which do not strike the earth, but only affect it by their attraction, the effect will be an exceedingly minute acceleration of the earth's motion, if the latter *is less than that of the bodies*, even though the group is infinite in extent. If the earth's velocity *is greater than that of the bodies*, their total effect will consist of two parts: a very minute retardation of the earth's motion, depending in amount upon the absolute velocity of the bodies; and another retardation depending upon the assumed extent of the group. In conclusion, the effect of bodies *striking* the earth or moon is manifold greater than that of those only *passing near*; and since it has before been shown that any admissible magnitude of meteroids would make the effect upon the moon's mean motion of those which strike it only a minute fraction of the observed acceleration, still less can any action of those passing near the moon have any appreciable effect.

Papers were also read by Prof. Harkness on the flexure of transit instruments; by Prof. Hough, describing some improvements recently introduced in the printing chronograph, first designed and brought into use by himself at the Dudley Observatory in 1871, by Prof. Burkitt Webb, describing a method of using polar coordinates, by transferring the origin from the centre to the end of the unit radius, thus substituting $(r-1)$ for r, and then using the length of the arc and the distance out from its end upon the radius vector, as x and y are used in rectangular coordinates. He found this a very convenient transformation in the application of polar coordinates to the discussion of Amsler's planimeter; and, pointing out, that by substituting infinity for unit-radius in the equations thus transformed, they were reduced to those of rectangular coordinates, he thought this transformation of polar coordinates might be found generally useful.

In this section also Mr. Rockwell presented some results of his observations for time and latitude with the almucantar, an instrument devised by Mr. Chandler, of the Harvard College Observatory, a year or two ago, which promises at least to furnish an entirely new and radically different method of attacking the question of absolute positions of the stars, and very probably far to surpass all others in accuracy, on account of its freedom from systematic errors. The results thus far published by Mr. Chandler seem fully to confirm all that was expected of the instrument; and it is probably not too much to say, that it is the most important addition of the present century to the instruments and methods used in the determination of absolute star positions.

The sources of systematic error would seem to be almost wholly reduced to those of varying personal equation in the observation of transits at all speeds and at all inclinations and directions over horizontal wires, and to possible systematic difference in atmospheric refraction in different azimuths. Mr. Rockwell exhibited some results, simply copied from his observing-books, illustrating the methods of reduction for time and latitude observations, and showing the degree of accuracy that can be attained by the instrument in both these directions. They served to show that the instrument when duplicated will give equally good results with the one first constructed; and their consideration gave rise to a very interesting discussion, participated in by many members, as to the character of work the instrument might be expected to do, in the course of which Mr. Rockwell answered, in a very entertaining way, many questions, put by various members, as to the details of observing and reducing, which were not before clearly understood on account of the novelty of the work. One of the most important problems which the instrument is specially adapted to investigate, and one which we hope Mr. Chandler will soon find time to undertake, is the determination of the declination of fundamental stars south of the equator, tying them to northern stars at corresponding zenith-distances below the pole. This would seem to be by far the best, perhaps the only, method of connecting these together in a way that shall be free from systematic error.

In the Physical Section, the first paper read was by Prof. Langley, on the spectra of some sources of invisible radiations, and on the recognition of hitherto unmeasured wave-lengths. This was followed by one by Mr. Brashear on a practical method of working rock-salt surfaces for optical purposes.

Prof. H. S. Carhart presented a paper on surface transmission of electrical discharges, which was an ingenious revision of work by Prof. Henry. Prof. E. L. Nichols presented some further notes on the chemical behaviour of magnetic iron, a continuation of work described in a paper at the Philadelphia meeting. Major H. E. Alvord of Mountainville, New York, presented the results of telemetric observations at Houghton Farm. This is a method by which changes in temperature are transmitted and recorded electrically; and Major Alvord's results show that, with increasing experience, the records followed more and more satisfactorily the observations made on the mercurial thermometer.

Prof. T. C. Mendenhall called attention to the modifications and improvements already made or desired in electrometers, especially with reference to their use in observations on atmospheric electricity. Observations of this kind have been made regularly for the last year or two; but, as Prof. Mendenhall well said, the meaning of the variations recorded is still a mystery. Prof. A. E. Dolbear read three papers: in one he described a method of studying contact-theory of electricity by means of the telephone. He has found that a click is produced in the telephone every time the circuit is broken between two heterogeneous materials, as copper and zinc. In another paper he referred to his success in employing a Bernstein incandescent lamp for projection purposes; and in the third he described a new galvanic element of high electromotive force and great constancy, consisting of carbon in a saturated solution of bichromate of potash, and sulphuric acid and zinc in a saturated solution of ammonic chloride; nitric acid could be used in place of sulphuric. Mr. A. J. Rogers presented a paper on electrolysis of the salts of the alkaline earth.

Prof. E. D. Nicholls has, by means of a spectro-photometer, described at a previous meeting, compared the spectrum of the unclouded sky with that of the light reflected by magnesium carbonate, illuminated by direct sunlight.

Prof. Wead exhibited a combined spectro-photometer and ophthalmospectroscope.

In the Chemical Section Prof. Nichols delivered an address on chemistry in the service of public health. Amongst the papers are:—Prof. Noyes, on para-nitrobenzoic sulphuride; Dr. Wiley, on a method of estimating lactic and acetic acid in sour milk or *koumiss;* Mr. Young, on the thermo-chemical reaction between potassic hydrade and common alum. A general discussion took place on the question of what is the best initiatory work for students entering upon laboratory practice, and also, To what extent is a knowledge of molecular physics necessary to one who would teach theoretical chemistry?

In the Section of Mechanical Science Prof. Webb delivered an address on the second law of thermo-dynamics. Mr. Wagner presented an elaborate paper on electric light tests, giving an account of his work in testing the efficiency of two electric-light plants. Prof. Cooley explained and illustrated a method of testing indicator-springs. Prof. Thurston's paper on cylinder condensation is described as being of great scientific and practical value.

In the Section of Geology and Geography the address was by Prof. Edward Orton, and the subject, Problems in the study of coal, with a sketch of recent progress in geology. There were, in all, twenty-seven papers in this Section, none being geographical. Stratigraphy received the lion's share of attention, the most important paper on this subject being one by Prof. Henry S. Williams.

The address to the Biological Section was by Dr. Wilder, on Educational Museums of Vertebrates.

The Section opened with two papers by Prof. L. E. Sturtevant as the result of observations and experiments at the New York agricultural experiment station. The first, on the hybridisation and cross-fertilisation of plants. In the second—"Germination Studies"—the author gives, as a result of many trials with commercial seeds of our common plants, that very extended series of trials must be made with each species in order to obtain the desired accuracy in results.

An interesting paper on the biological deductions from a comparative study of the influence of cocaine and atropine on the organs of circulation, by Dr. H. G. Beyer, U.S.N., was read before the Section.

"On the Brain and Auditory Organs of a Permian Theomorph Saurian" was the title of an interesting paper by Prof. E. D. Cope. The author called special attention to the morphology of the brain, the character of the cranial walls and the auditory apparatus.

The disputed question of the bisexuality of the pond-scums (Zygnemaceæ) was discussed by Prof. C. E. Bessey, of the University of Nebraska, who concluded that these organisms do not possess true bisexuality.

"On the Process of Cross-fertilisation in *Campanula americana*" was the title of a paper presented by C. R. Barnes.

A paper on aquatic respiration in soft-shelled turtles (Aspidonectes and Amyda) was presented by Profs. Simon H. and S. Phelps Gage as a contribution to the physiology of respiration in vertebrates.

Prof. C. E. Bessey read a paper on the inflorescence of *Cuscuta glomerata*.

Prof. Gage addressed the Section (G) on Microscopy and Histology on the limitations and value of histological investigation, and Mr. Dall discoursed to the Anthropological Section on the native tribes of Alaska. The papers in this section were very numerous, many of great interest, and all naturally devoted to anthropological questions connected with the North American continent.

NOTES

THE National Sanitary Congress commenced its autumn meeting at Leicester on Tuesday, when the president, Prof. De Chaumont, F.R.S., gave an address on the work of the Sanitary Institute.

THE portrait of the late George Bentham, subscribed for by several of his friends, has been presented to the Herbarium, Royal Gardens, Kew, on behalf of the subscribers, by Sir John Lubbock. The picture is a successful reproduction, by Miss Merrick, of the original in the possession of the Linnean Society.

WE regret to notice the death of M. Breton des Champs, one of the French Government engineers, a mathematician and scientific writer who played a prominent part in connection with the Newton forgeries. In combination with his friend Leverrier, M. Breton des Champs exploded these frauds, which were so disgraceful to the good name of the French Academy of Sciences. He discovered the books from which the so-called "forger with long ears" had copied the assumed letter sold to M. Chasles.

THE Essex Field Club will hold its sixth annual cryptogamic and botanic meeting in Epping Forest on Friday and Saturday, October 2 and 3. On the Saturday afternoon and evening there